surveyors and astronomers. The examples are mostly taken from actual observations, and the necessary calculations are given in considerable detail. In the earlier chapters a knowledge of determinants is not assumed, and the explanations given ought to make the method intelligible to readers of quite moderate mathematical ability. For the more difficult and controversial points of the theory, reference is made to the treatise of Czuber; at the same time, a very good example of the unavoidably empirical nature of the whole subject is given by working out the same elementary problem according to each of three different laws of error. Now that the measurements of physics and chemistry are approaching, not to say surpassing, in exactness those of astronomy and geodesy, a practical work of this kind is likely to assist a larger and larger body of experimenters.

Die Purpurbakterien. Eine mikrobiologische Studie. By Prof. Hans Molisch. Pp. 92. (Jena: Gustav Fischer, 1907.) Price 5 marks.

THIS memoir deals with an interesting group of chromogenic microorganisms, viz. those producing brilliant pigments ranging in tint from pink, through rose and deep red to reddish-purple. They are probably more nearly allied to the coloured algæ (Phycochromaceæ) than to the bacteria proper, and one of the earliest descriptions of a member of the group was given by Sir Ray Lankester in 1873 under the title of a "peach-coloured bacterium." The author first discusses the occurrence in nature of these organisms. Sometimes they are met with in great abundance on the sea-coast, in river estuaries, and in hot and sulphur springs. Directions are given for obtaining growths in various organic mixtures, for the preparation of suitable culture media, and for obtaining pure cultures in the latter. The biological and physiological properties are next considered; while light has an inhibitory, or a germicidal, action on most bacteria, the "purple" bacteria develop best in its presence. They are sensitive to all light rays, but in particular to the ultra-violet ones; they do not, however, evolve oxygen in the presence of light, and their need for oxygen varies much, some species being almost anaërobic.

The colouring matter produced by the "purple" bacteria is a mixture of two pigments—a green, "bacteriochlorin," and a red, "bacteriopurpurin." The chemical and other properties of these are described fully.

As regards classification, the organisms are grouped in a special order, the Rhodobacteria. This is divided into two families, distinguished by the presence or absence of sulphur granules, and several new species isolated by the author are described. Altogether the book forms a very useful summary of our knowledge of an interesting and peculiar group of microorganisms. R. T. HEWLETT.

The Case of Existence. By Norman Alliston. Pp. xiii+262. (London: Kegan Paul, Trench, Trübner and Co., Ltd., 1907.) Price 5*s.* net.

"OF the enigmas of life," says Mr. Alliston in his introduction, "all speak; but nobody acts as if there were an enigma." It is his object to remove this inconsistency by exposing the confusions of thought of those who "want life speculatively to be a mystery." The book has three parts. The first contains a good account of the nature of Explanation and some not ineffective criticisms of Knowledge, Nature, &c., mingled, it must be admitted, with much rather ill-informed dogmatism. The second, in the course of a review of man's "obstinate questionings" about existence, develops the author's peculiar egoistic optimism. The third, in which he draws his ethical corollaries, unfortunately contains some chapters which many readers will find offensive both in matter and in tone. The book contains little to engage the attention of the practised student of philosophy, but, being written with obvious conviction and enthusiasm, may here and there attract a useful recruit to the study of first principles. At a later stage the student may not unprofitably return to these pages to detect and analyse the crudities and ambiguities which abound under a surface of apparent lucidity.

Science German Course. By C. W. P. Moffatt. Pp. xii+228. (London: W. B. Clive.) Price 3*s.* 6*d.*

THE portion of this book devoted to grammatical construction and word formation occupies about eighty pages, and is followed by extracts for translation from the German. These selections deal with various scientific subjects, and can be commenced after the student has made himself familiar with the first few pages of the grammar that precedes them. Short vocabularies are given of technical terms in mathematics, physics, chemistry, geology, botany, and zoology. The book thus provides a convenient means of obtaining sufficient acquaintance with the German language to read simple scientific descriptions in it with intelligence.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Wehnelt Kathode in a High Vacuum.

THAT a good vacuum can be made into a good conductor by the use of an incandescent kathode is known since the discovery of the Edison effect, and has been investigated with great thoroughness by O. W. Richardson (Trans. Roy. Soc., 1903, 201 A, 497). Wehnelt has shown (*Ann. d. Physik*, 1904, iv., 14, 425) that if the incandescent kathode is coated with one of the alkaline earths, surprising results can be obtained. From a platinum foil kathode at 1300° C. to 1400° C. coated with lime, two to three amperes per sq. cm. of surface can be passed through a good vacuum, the kathode fall being practically negligible, and the total voltage across the vacuum tube being below 30 volts. This result is so very remarkable that I have repeated it in the following way to test whether, as is commonly supposed, the phenomenon is really independent of the perfection of the vacuum.

In a tube provided with a Wehnelt kathode of about a sq. cm. area was mounted an anode of the metal calcium. I have recently shown (Proc. Roy. Soc., 1907, 78 A, 429) that calcium at its volatilising temperature (700° C. to 800° C.) absorbs practically instantaneously and very perfectly all known gases and vapours except the chemically inert gases of the argon family, and have described a form of vacuum furnace suitable for this operation. The tube was prepared in the usual way by preliminary exhaustion and washing out with oxygen to remove argon, and then subjected to the action of calcium heated in a furnace attached to the apparatus. When a good vacuum had been obtained, current from the 250-volt supply was passed through the tube between the heated Wehnelt kathode and the calcium anode in order to heat the latter.

The gases evolved from the anode and tube under this treatment were absorbed by the calcium in the furnace. The current was regulated by a resistance to about 1·2 amperes, and was interrupted at intervals to give the evolved gases time to flow out of the apparatus. When the gases had been for the most part removed the current was passed continuously, heating the calcium anode up to its volatilising point. Quite suddenly and completely the current through the tube stopped, and at the same moment a copious mirror of calcium was volatilised from the anode. In a little while a very feeble glow started and passed

intermittently from time to time, but it was not enough to move the ammeter needle, and was most likely due to a further slow evolution of gas from the still heated surfaces. A current could be passed by a coil from the hot kathode to a third electrode as anode without causing any appreciable resumption of the flow in the 250-volt circuit.

More gas was then generated within the apparatus by heating the third electrode with the coil discharge, and the current in the main circuit resumed its original intensity, again heating up the calcium anode. The original phenomenon was repeated, a sudden cessation of current taking place when the calcium volatilised. Just before stopping, the glow of the tube changed to that characteristic of argon, so probably a trace of air had not been removed. The whole phenomenon could be repeated by admitting oxygen to the apparatus and proceeding as before.

This experiment shows that in a sufficiently high vacuum the Wehnelt electrode ceases to be effective. In the experiments so far recorded the saturation current has increased with the improvement of the vacuum, and the phenomenon has been supposed to be in the first place independent of the residual trace of gas present. Wehnelt (*loc. cit.*, p. 445) remarks:—"Für Drucke unter 0·1 mm. ist die für eine bestimmte Temperatur ausgesandte Zahl von negativen Ionen unabhängig vom Druck," and (p. 456) "die Grenzstromstärken um so höher sein . . . je tiefer die Druck ist." In his description of his modification of the Braun tube (*Phys. Zeit.*, 1905, vi., 732) he says the vacuum in the tube must be as perfect as possible.

Richardson, whose mathematical theory of the general phenomenon has received quantitative experimental confirmation, and has been accepted by Wehnelt in the case of his electrode, regarded the action as purely electronic. Commenting on the magnitude of the current and the smallness of the residual gas—in one experiment 2 amperes per sq. cm. at a measured pressure of 0·0016 mm. from a carbon lamp-filament—he says (*loc. cit.*, p. 546):—"This (the current) is twenty-five times the maximum value obtained by supposing each molecule to produce one ion; so that it is highly improbable that any considerable part of the conductivity investigated is due to ions produced in this way. . . . Both these points of view lead to the conclusion that the corpuscles are not produced by a dynamical action between the molecules of the surrounding gas and the surface of the metal. In fact, all the experimental results seem to point to the view that the corpuscles are produced from the metal by a process similar to evaporation."

These isolated quotations, of course, may not fairly express the opinions of the authors about what is a very complex phenomenon; but the general impression their results has conveyed, I think, has been that the large currents dealt with were wholly conveyed by the expelled electrons, and therefore should pass through any vacuum, however perfect. I do not think the electronic emission can account for more than a negligible fraction of the total current, which is carried almost wholly by the residual gas.

The results here given bear out the general view I have from time to time advocated since my experience with the use of calcium, that degrees of vacuum are in practice apt to be overrated, and really high degrees of vacuum are not so readily obtained as is commonly supposed.

FREDERICK SODDY.

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The Interpretation of Mendelian Phenomena.

I AM strongly inclined to agree with Dr. Archdall Reid that Mendelian investigations throw no light on many of the most important problems of biology, such as the causes of variation, the evolution of adaptations, and many others. On the other hand, it is difficult to understand what Dr. Archdall Reid means by the statement that Mendelism is the investigation of sex. In one of his letters he describes Mendelian phenomena as "abnormalities of sexual reproduction which occur under conditions of artificial selection." If this means peculiarities of heredity in sexual reproduction observed in cultivated varieties, it may be allowed to pass; but in an appendix to the second edition of his "Principles of Heredity," Dr. Reid states that the inheritance of Mendelian characters is probably sexual. He proceeds as follows:—"Nature has evolved alternative inheritance to create and perpetuate sexual differentiation, but, just as blending of sexual characters sometimes occurs, so on the other hand the inheritance of non-sexual characters is sometimes alternative. As we have seen, whenever the latter happens, the non-sexual differences are, like the sexual differences, usually considerable. Nature makes the mistake, so to speak, of treating them as sexual. Of course, however, the alternative inheritance of large non-sexual differences is not certain, not so 'clean' as that of sexual differences, the alternative inheritance of which has been established by a long course of selection."

According to this, the alternative inheritance of non-sexual characters is of the same nature as sexual dimorphism. It seems to me that Dr. Reid has not sufficiently studied the inheritance of secondary sexual characters. It is admitted by almost all evolutionists from Darwin himself downwards that selection cannot have been the cause of the alternative inheritance of sexual differences. The female in selecting a particular male cannot prevent the transmission of his peculiarities to her daughters. The male characters must be limited to the male sex from their first origin as variations, because otherwise the selection of the individual male would merely ensure their transmission to both sexes.

The essential peculiarity of secondary sexual characters is their physiological connection with the primary, *i.e.* with the male or female gonads. It is not merely a question of alternative inheritance; both male and female characters are inherited by each individual, but normally only one set is developed. When, however, the male gonads are removed, the male characters are usually not normally developed, but suppressed. In Mendelian cases the development of alternative characters is usually entirely independent, both in theory and fact, of the sex or condition of the gonads.

The contrast of male and female corresponds to alternative dominance in a heterozygote; male characters are dominant in male, female in female, but either can and does transmit both. In Mendelian cases alternative inheritance is segregative; a recessive transmits only recessive characters, a pure dominant only the dominant. In other passages which I could quote Dr. Reid has shown that he has failed to appreciate this fundamental distinction between dominance and segregation, between the heterozygote and the homozygote, between the first generation of a Mendelian cross and the second.

Excepting parthenogenesis, the heredity of all characters is sexual in that it is connected with fertilisation, but I do not see that Mendelian characters are sexual in any other sense. The theory that the inheritance of sexual characters is Mendelian is one which has a definite meaning and can be investigated. Dr. Archdall Reid's statement that Mendelian inheritance is sexual is in one sense a truism, in another seems to me to have no real meaning at all, for to say that nature has evolved alternative inheritance to perpetuate sexual differentiation explains nothing.

J. T. CUNNINGHAM.

Highgate, November 3.

I AM very willing, and, like most people with theories, I believe I am able to maintain the correctness of the views to which Mr. Cunningham refers; but to discuss my speculations now would be to confuse the issue. I may say, however, that the appendix to the second edition of my work, from which Mr. Cunningham quotes, is a mere sketch hastily thrown together to meet the objections of critics who had advised the lay and scientific public that a book, which I fondly hoped contained a little that was new, and which certainly contained more than a little of which Mendelians seem profoundly unaware, was antiquated and worthless, not because there were no new facts or inferences in it, nor because its facts and inferences were invalid, but simply and solely because I had not adopted "the new method" nor accepted the